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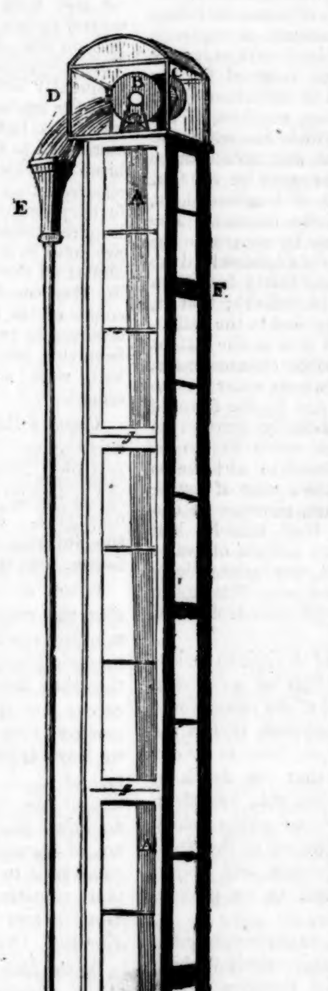
THE HYDRAULIC BELT.

The machines for raising water are so numerous, and act on so many different principles, that the term "Hydraulic machine" is capable of many different acceptations. In general, however, a system of buckets, attached to the periphery of a wheel or to the links of a chain, furnishes the means whereby water may be raised from one level to another.

Within a few years a new mode of effecting this transfer has been adopted, viz., by allowing a fibrous material to dip into water, and then to ascend, by which means it conveys away a certain quantity of water which had clung to the fibres, or had lodged in the interstices between them, by capillary attraction. We all know that if a piece of string or of cloth be dipped into water and then lifted out again, a portion of the water remains adhering to the cloth or string, in opposition to the gravitating tendency; and, indeed, this is the reason why such articles become *wetted;* the wetting of the fibres being the effect of capillary attraction. If we coil up a few fibres of cotton, place one end in a basin of water, and allow the other end to lie on the table, the water in the basin, by the operation of capillary attraction, will ascend the minute channels between the fibres of the cotton, pass over the edge of the basin, and flow out at the lower end of the cotton. If the outer end of the cotton be altogether below the level of the bottom of the basin, the transfer of water through the little capillary tubes of cotton will go on till the basin is entirely emptied; for the cotton may be deemed an assemblage of *siphons,* whereby water is conveyed from one spot or level to another situated somewhat lower.

This capillary, absorptive, or succulent action of fibrous materials on water, has been employed as a means for raising water from one level to another. Some years ago a *Rope pump* was used for such a purpose. It consisted of two, three, or more hair ropes, passing over pulleys fixed at the top and bottom of the well, or receptacle, from which the water was to be elevated. The ropes were about an inch apart; and when the pulleys were made to revolve, the ropes carried up with them a column of water, which by a peculiar contrivance was deposited in a reservoir at the top. It is asserted, that a pump of this kind has been known to raise nine gallons of water in a minute, from a well ninety-five feet deep, by the efforts of one man.



WORKING MODEL OF THE HYDRAULIC BELT.

Similar in principle is the Hydraulic Belt, or Elevator, invented by Mr. Hall, and intended, we believe, partly for draining fens and marshes, as well as for the raising of water generally. The form in which the instrument is constructed may be varied according to circumstances:—the working model, deposited in the Polytechnic Institution, shows very clearly the principle on which this instrument acts. The annexed cut represents its appearance: it is deposited in a glass-case, F F, to prevent the water from being scattered about after having been raised by the belt.

The machine consists of an endless woollen band or belt, passing over two plain rollers, one placed in the water to be elevated, and the other at the spot where the water is to be discharged. A represents the belt, in the model, passing over B, the upper roller, and also over C, the lower roller. Both of these rollers are capable of revolving on their axes; and as the belt is stretched lightly over them, if one roller be made to revolve, the other roller revolves likewise; and the belt thus travels alternately up and down. When the upper roller, by the communication of power from any convenient source, is made to revolve with a certain degree of rapidity, the ascending band carries up a considerable quantity of water, which is discharged at D by the pressure of the belt on the upper roller, and passes into a funnel E, from which a pipe conveys it down to the tank below.

A belt acting on these principles was employed some time back in raising water from a well at the west end of London; and two experiments made with a view to test its powers gave the following results.—In the first experiment, from an average depth of a hundred and thirty feet, with a seven-inch woollen band, the

quantity of water discharged in about twelve minutes was eighty-six cubic feet, equal to about forty-five gallons per minute: in a second experiment, from the same depth and with the same band, fifty cubic feet of water were raised in about six minutes, averaging about fifty-one gallons per minute. It is stated that the most effective velocity of the band is about one thousand feet per minute, and that for every additional inch in the breadth of the band, there are from seven to eight gallons gained per minute. Calculations have been made to show what relation the effect bears to the power employed, in the hydraulic belt and in the common lifting pump; and in reference to this subject the following remarks have been made:—

For thousands of years the brains of philosophers and discoverers have been racked in the pursuit of means for lifting water, wherein the greatest possible amount of capacity, simplicity, and economy should be combined: odd as it may appear—and it does appear odd—it was reserved for the present age to perfect, and bring forward an invention which far surpasses, in these important requisites, anything of the kind previously known; an invention which has withstood the test of experiment under every form and circumstance of disadvantage, to which new things brought for the first time into practical use, without the aid of lengthened, or, indeed, of any experience, are necessarily exposed. The *power* which the water-elevator possesses by nature is one of the most extraordinary and least easily explained things about it. A common pump will lift water thirty feet at an expenditure of 100 (power) to produce 60 (effect); that is, for every 100 pounds mechanical force applied to the piston, 60 pounds of water will be raised; and this is the extent of its capacity under the most favourable circumstances. But in the case of a force or lift-pump, where water has to be carried above the height of thirty feet by the force of compression, this per-centage will materially decrease in proportion to the height to which the water has to be elevated. This part of the subject appeared to us to be so all-important and absorbing, that we made a point of having an experiment tried at our own expense, in order to discover if the statement made by Mr. Hall, that his belt would lift at great depths from 85 to 90 pounds of water for every 100 pounds of power employed, was substantially correct. The result of that experiment was, that steam power equal to 107,892 pounds lifted 96,460 pounds of water, or nearly 90 per cent.

Other writers have stated the effect of the belt to be less than this, although still higher than that of a common pump; for instance, that when the effect of the pump would be about 54 per cent. of the power employed, that of the belt would be 74 per cent; being 20 per cent. in favour of the belt. It is stated, however, that the depth at which these experiments were carried on, viz., 130 feet, gave an advantage to the belt over the pump which would not occur at smaller depths, owing to the great absorption of moving power by the friction and weight of the pump-rods necessary to descend to so great a depth.

It seems evident that more accurate experiments must be made in order to determine the real relation which the power of the belt bears to that of a common lifting pump; but, at the same time, opinions appear to be favourable towards the former, as a mode available under particular circumstances. Finally, we may observe that it is supposed that the water is not retained in the belt simply by capillary attraction; but that at high velocities, the air tends to press the water close to the belt, and to retain it there during the ascent.

WHEN from my humble bed I rise,
And see the morning sun,
Who, glorious in the eastern skies,
His journey has begun,
I think of that Almighty Power,
Which called his orb from night;
I think how many at this hour
Rejoice beneath his light.

And then I pray in every land,
Where'er its light is shed,
That all who live may bless the Hand
Which gives their daily bread.—BOWLES.

RED SNOW.

IN speaking of snow we always associate with that substance the idea of pure and dazzling whiteness; and it is therefore somewhat difficult to believe that such a phenomenon as *red snow* can possibly exist in nature. Yet we have the testimony of men of undoubted veracity that such is the fact. Saussure observed it on Mount Breven, in Switzerland, in 1760. Ramond found red snow on the mountains of the Pyrenees, as did Sommerfeldt on those of Norway. Captain Parry, in his Arctic expedition, also noticed this remarkable appearance He says:—

In the course of our journey on the 2nd of August (1827) we met with a quantity of snow tinged, to the depth of several inches, with some red colouring matter, of which a portion was preserved in a bottle for future examination. This circumstance recalled to our recollection our having frequently before, in the course of this journey, remarked that the loaded sledges, in passing over the hard snow, left upon it a light rose-coloured tint, which at the time we attributed to the colouring matter being pressed out of the birch of which they were made. To-day, however, we observed that the runners of the boats, and even our own footsteps, exhibited the same appearance; and on watching it more narrowly afterwards, we found the same effect to be produced in a greater or less degree by heavy pressure, on almost all the ice over which we passed, though a magnifying glass could detect nothing to give it this tinge. The colour of the red snow which we bottled, and which only occurred in two or three spots, appeared somewhat different from this, being rather of a salmon than a rose-colour, but both were so striking as to be the subject of constant remark.

Captain Ross subsequently described this red snow as existing on Arctic mountains which were six hundred feet high, and eight miles in length. The depth to which the red tinge extended was differently stated by different observers. Some found it to reach many feet beneath the surface, others never ascertained that it spread beyond one or two inches.

It was at length considered as an ascertained point that this rosy hue was caused by a vast assemblage of minute vegetable bodies, belonging to the class of cryptogamic plants, and the natural order called *Algæ*, and that they formed the species, named by Agardh, *Protococcus nivalis*. But though this is true of a small portion of the bodies to which the red tinge is due, yet we learn from the more recent researches and discoveries of R. J. Shuttleworth, Esq., that the great proportion of the red snow of Alpine districts, (and therefore doubtless that of the Arctic region also) is of *animal*, not of *vegetable* origin. This will be best shown if we endeavour to simplify the scientific description given by that gentleman in the *Bibliothèque Universelle de Genève*, and also in the *Edinburgh New Philosophical Journal*, 1840.

On the 25th August, 1839, (says Mr. Shuttleworth,) being at the *Hospice du Grimsel*, I learned that some patches of snow in the neighbourhood were beginning to acquire a red tint. The weather for some days previous had been very bad; and quantities of snow had actually fallen, which at the same time soon began to melt under the influence of milder weather, and of warm rains. The 24th was a day of thaw and mist; the 25th was clear, the temperature being agreeable, and even hot in the sun, the gentle breeze which prevailed being by no means cold. Accordingly I hastened to visit the spot, accompanied by my friend Dr. Schmidt, and by MM. Muchlenbech, Schimper, Bruch, and Blind, distinguished Alsace naturalists, who that day, to my agreeable surprise, arrived at the Grimsel.

It was in those places where the snow never entirely melts that we found the patches in which the red snow was beginning to appear. The patches were somewhat inclined, with an exposure towards the east and north-east. Their surface was more or less bestrewed with small earthy particles which gave them that dirty grey appearance which old snow always presents at inferior heights, and in positions which are overlooked by more elevated ground. The surface was moreover furrowed, and slightly hollowed out

owing to the effects of the wind, and the run of water produced by the partial thaw on the surface, which was much promoted by the great absorption of heat by the earthy particles. Here and there spots were remarked of a rosy hue, or of the colour of very pale blood, whose form and extent were indeterminate, but which were most conspicuous in the furrows and hollow places. Old snow being always more or less coarsely granular in its nature, we observed that the colouring matter was contained in the intervals between the particles, and this gave to the surface when viewed near, somewhat of a veined appearance. The coloured spots descended beneath the surface of the snow to the depth of several inches, and often almost a foot; sometimes the colour was most conspicuous on the surface, but at other times it was most remarkable some inches below it. Whenever rocks or stones had occasioned little wells in the snow, the perpendicular sides of these wells were also coloured to the depth. On the whole, however, the colouring matter penetrated only to a very trifling extent into the substance of the snow, which became more and more compact in proportion to its distance from the surface.

A sufficient quantity of this coloured snow having been collected, and placed in vessels of earthenware, was at length subjected to microscopic examination. As the snow melted, the colouring matter became gradually deposited upon the sides and bottom of the vessels, under the form of a deep red powder. After two or three hours, the snow being partially melted, a portion was introduced under a microscope of the power of 300 diameters. Mr. Shuttleworth was not a little astonished to find that the colouring matter was composed of organized bodies of different forms and natures, some of which were vegetables, but by much the larger proportion, endowed with swift movements, belonged to the animal kingdom. The colour of the greater number was a bright red, approaching sometimes to a blood colour, at other times to a crimson, or a very deep brown, and almost opaque red. Besides these coloured bodies, there were others, either colourless, or greyish, the largest of which were of an animal nature, and so few in number, that it was suspected their presence was accidental, and the smaller, evidently vegetable, filled up all the spaces unoccupied by the others.

The most striking of the bodies thus discovered, and those which, from their great numbers, and deep colour, mainly produce the red tint of the snow, were small *infusoria** of an oval form, whose colour was a very deep reddish brown, and which were nearly opaque. These creatures moved with astonishing rapidity, and in all directions. The majority were of a perfectly oval form; but some were pear-shaped. The former moved with an equal horizontal motion, but the latter often stopped in the middle of their course, and turned rapidly round on their pointed extremity without changing their places. In the oval bodies, one or two reddish and nearly transparent spots were observed either in the centre, or near one of the extremities: these Mr. Shuttleworth regarded as stomachs. Besides these spots, he could discover no sign of organization. This species is named by Mr. Shuttleworth ***Astasia nivalis***.

Among these infusoria, there were a few much larger than the rest, and differing from the above in being of a beautiful blood-red colour, inclining to crimson, and to a considerable degree transparent. They were round or oval in shape, and were surrounded with a margin, or colourless membrane. In these Mr. Shuttleworth could not observe any movement, or the slightest trace of internal organization; but he has no doubt that they are likewise infusory animals, and considers them to be a species of ***Gyges***, to which he gives the name ***Gyges sanguineus***.

A small number of still more minute bodies were likewise found under the microscope: they were perfectly spherical, and of a beautiful blood-red colour, though somewhat transparent. Viewed in certain positions, they exhibited at one of their edges a small cleft or very narrow opening. Their movement was progressive, and in circles, and they turned upon their axes at the same time. There were others, perfectly spherical, of a crimson colour, slightly transparent at their edges, and surrounded with a membrane which was without colour. At one determinate point, towards the edge, the colouring mass exhibited an opening, which was transparent, and almost colourless, in the shape of a half moon, and which communicated with the membranous border. No motion was observed in these bodies, nor can it be determined with certainty to which genus to refer them.

Mr. Shuttleworth also describes the vegetable bodies which he found invariably to accompany the infusory animals, and which were distinguishable by their imperfect degree of transparency, and by the colouring matter escaping, under pressure, in the form of infinitely small granules.

The existence of this remarkable fact (says Mr. Shuttleworth), which I believe has not hitherto ever been suspected, viz., that in the red snow, there exists an infinite number of microscopic beings, which are evidently animals, and at a temperature rarely elevated more than a few degrees above the freezing-point, and often probably far below it, shows how much yet remains to be discovered in this new world, the limits of which will be extended in proportion as our microscopes become more perfect.

* The infusory animals, or *Infusoria*, were originally so called by Muller, a Danish naturalist, from the circumstance of their swarming in all *infusions* of vegetable or animal substances which have been kept for a sufficient time. They are so minute as only to be discovered by means of the microscope.

Merrily and wittily said Plautus, who was one of the merry wits of his time, I would (said he,) by my will, have tale-bearers and tale-hearers punished, the one hanging by the tongue, the other by the ears. Were his will a law in force with us, many a tattling gossip would have her *vowels* turned to *mutes*, and be justly tongue-tied, that desires to be tied by the teeth at your table: wherewith Thominus his tooth she knaweth on the good name of her neighbour; many a hungry parrot whose belly is his arts-master, would cease to second his *ave* to his lord with depraving tales called news, and make his grace after dinner the disgrace of some innocent: and most men would give them coarse entertainment, that come to entertain their ears with discourse of defamative reports. I will be silent and barren of discourse when I chance to hear a tale, rather than go with child therewith, till another's ears be my midwife, to deliver me of such a deformed monster. I may hear a tale of delight, and perhaps smile at an innocent jest; I will not jest nor joy at a tale disgracing an innocent person.

When I see a gallant ship well rigged, trimmed, tackled, manned and munitioned with her top and top-gallant, and her spread sails proudly swelling with a full gale in fair weather, putting out of the haven into the smooth main, and drawing the spectators' eyes, with a well wished admiration, and shortly hear of the same ship splitted against some dangerous rock, or wrecked by some disastrous tempest, or sunk by some leak sprung in her by some accident, meseemeth I see the case of some court-favourite, who to-day like Sejanus dazzleth all men's eyes with the splendour of his glory, and with the proud and potent beak of his powerful prosperity cutteth the waves and plougheth through the praise of the vulgar, and scorneth to fear some remora at his keel below, or any cross-winds from above; and yet to-morrow, on some storms of unexpected disfavour, springs a leak in his honour, and sinks on the Syrtes of disgrace, or dashed against the rocks of displeasure is splitted and wrecked in the Charybdis of infamy, and so concludes his voyage in misery and misfortune. I will not therefore adventure with the greedy shepherd to change my sheep into a ship of adventure, on the sight of a calm sea. I will study to deserve my prince's favour, I will not desire to be a prince's favourite. If I fall whence I am, I can raise myself; but to be cast down thence, were to be crushed with a desperate downfall. I prefer a mediocrity, though obsecure yet safe, before a greater eminency with a far greater danger.

[Arthur Warwick, 1637.]

HISTORICAL NOTICE OF THE Celebrated Latin Grace, *NON NOBIS, DOMINE.*

ANY person who has attended a public dinner in England, or who has read an account of the proceedings on such an occasion, must have noticed that one particular piece of music is usually sung, immediately after the conclusion of the dinner. This is the composition termed "Non nobis, Domine." It is a "grace after meat," set to music, and harmonized generally for three voices; and is much celebrated by musicians for the skill with which the several parts are arranged. It is one of those peculiar forms of composition, called in musical language a "canon:" in which the second part is a kind of imitation of the first, and the third an imitation of the second; but the second does not commence till after the first, nor the third till after the second; and the skill of the composer is displayed in causing all the three parts to harmonize together. The ear is capable of detecting a similar melody or subject sung by all the voices, not simultaneously; and yet in such a manner as to produce harmony. Without professing to go far into the subject, we will give a few bars of the canon, as an illustration of the principle involved in these compositions:—

The words are exceedingly simple, comprising nothing but the following:—"Non nobis, Domine, non nobis, sed nomini Tuo da gloriam;" which is the Latin form of the Scriptural passage:—"Not unto us, O Lord, not unto us, but unto Thy name give the praise!" The air comprises but thirty bars; yet it is one which foreigners have been desirous of claiming for one of their own composers; for this, like "God save the Queen," and the "Hundredth Psalm," is one of those tunes whose affiliation is not easily determined.

English writers on musical history pretty generally agree that William Bird, or as the name is sometimes spelt, Byrd or Byrde, was the composer of this canon; and the evidence in favour of this opinion seems to be more conclusive than that by which the claims of a foreigner are supported. Sir John Hawkins, in his *History of Music*, remarks:—

There seems to be a dispute between us and the Italians, whether the canon "Non nobis, Domine," be of the composition of our countryman Bird or of Palestrina. That it has long been deposited in the Vatican Library, and there preserved with great care, has been confidently asserted, and is generally believed; and that the opinion of the Italian musicians, that it was composed by Palestrina, may be collected from this, that it has lately been wrought into a concerto in eight parts, and published at Amsterdam, in the name of Carlo Ricciotti, with a note that the subject of the fugue of the concerto is a canon of Palestrina: and that subject is evidently the canon above mentioned, in all its three parts.

Hawkins proceeds to express an opinion, that though it is admitted that the canon "Non nobis, Domine," does not occur among any of the works of William Bird, and also that its first publication was by John Hilton in 1652, yet that the evidence in favour of Bird is much more conclusive than that in support of Palestrina. He remarks that in such a case *tradition* may be allowed to have some weight; and that it is hard to conceive how a falsehood, if it be such, like this could have gained credit, and have remained undisturbed for two centuries. Dr. Pepusch, in his Treatise on Harmony, has expressly ascribed it to Bird; and most English composers seem to rest satisfied that to him and him alone the honour of the composition is due.

Dr. Burney, too, in his *History of Music*, published about the same time as the history by Hawkins, offers a similar opinion. He remarks, while speaking of Bird's compositions:—

The canon "Non nobis, Domine," appears in none of his works published by himself or collected by others, before the year 1652; when Hilton inserted it and prefixed the name of Bird to it, in a collection of catches, rounds, and canons. But as no claim was laid to it by, or in favour of, any other composer before or since that time, till about the middle of the present century (*i.e.* 1750), when it was given to Palestrina by Carlo Ricciotti, who published in Holland, among his concertos, a fugue in eight parts, upon the same subject,—there seems no doubt remaining of our countryman Bird having been the author of that pleasing and popular composition.

Burney proceeds to remark, that Zerlius, Palestrina, and many others among the old Italian composers, have made the same series of sounds, which form the burden of "Non nobis, Domine," the subject of incidental points in their compositions; but he adds, that he has never been able to discover a regular canon on the same motive or subject.

Hawkins and Burney evidently agree in believing Bird to have been the composer of "Non nobis, Domine," and we may therefore fittingly notice a few points in the career of this musician. William Bird is supposed to have been the son of Thomas Bird, who was one of the Gentlemen of Edward the Sixth's Chapel. William was at an early age one of the singing boys at this chapel. By the great number of his ecclesiastical compositions to Latin words, and the several portions of the Romish ritual which he frequently set to music and published late in life, he seems to have been long a zealous adherent to that religion. It appears probable, however, that he afterwards embraced the Protestant religion, since he filled the office of organist of Lincoln Cathedral from 1563 to 1569, and was subsequently one of the Gentlemen of the Chapel Royal.

Bird was a very voluminous composer; anthems, services, responses, psalms, sonnets, songs, fantasias, fugues, concertos, all received a portion of his attention, and most of his compositions have been highly valued by subsequent composers. He published one collection called:—"Psalms, Songs, and Sonnets; some solemne, others joyful, framed to the Life of the Words, fit for Voyces or Viols, of three, four, five, or six partes." Another collection was called:—"Songs of sundrie Statures, some of Gravitie, and others of Myrth, fit for all Companies and Voyces." A third collection, called "Psalms, Sonnets, and Songs, of Sadness and Pietie," contains the following curious preface.

Reasons briefly set doune by th' Auctor, to persuade every one to learn to sing:—

1st. It is a knowledge easily taught, and quickly learned, wher there is a good master and an apt scoler.

2nd. The exercise of singing is delightful to nature, and good to preserve the health of man.

3rd. It doth strengthen all parts of the breast, and doth open the pipes.

4th. It is the best means to preserve a perfect pronunciation, and to make a good orator.

5th. It is the only way to know where nature hath bestowed the benefit of a good voyce, which gift is so rare, as there is not one among a thousand that hath it; and in many that excellent gift is lost, because they want art to expresse nature.

6th. There is not any musicke of instruments whatsoever, comparable to that which is made of the voyces of men, where the voyces are good, and the same well sorted and ordered.

7th. The better the voyce is, the meeter it is to honour and serve God therewith; and the voyce of man is chiefly to be employed to that end.

Omnis Spiritus laudet Dominum.

Since Singing is so good a thing,
I wish all men would learn to sing.

We are very much inclined to join in this latter wish; and think, moreover, that there is much sound sense in many of William Bird's "Reasons." Bird, who died in 1623, seems to have been universally respected and admired by his contemporaries, many of whom have mentioned his name in a manner highly honourable to all parties.

Assuming that Bird was the composer of the canon "*Non nobis, Domine*," an attempt has been made to show that the tune was composed at the express instance of the Merchant Taylors' Company, in order to be sung at a Royal entertainment given by the Company. Mr. R. Clark, in his volume relating to the National Anthem, quotes a passage from the records of this Company, in which an account is given of a dinner at the Company's Hall, on July 16, 1607, at which King James the First and his son Prince Henry were present; the persons who assisted at the musical portion of the entertainment are also named, and among them is William Byrde. Mr. Clark then gives the following quotation from Stow's *Annals* in relation to the entertainment:—

The King, during this and the election of the new Maister and Wardens, stoode in a newe window made for that purpose; and with a gracious kingly aspect beheld all their ceremonies; and being descended into the hall to depart, his Majestie and the Prince were then again presented with like musique of voyces and instruments, and speeches, as at the first entrance. The musique consisted of 12 lutes, equally divided, 6 and 6 in a window on either side the hall, and in the ayre between them were a gallant shippe triumphant, wherein were three rare menne like saylors, being eminent for voyce and skill, who in their several songs were assisted and seconded by the cunning lutanist. There was also in the hall the musique of the city; and in the upper chamber the children of his Majestie's Chappell *sang a grace* at the King's table.

Mr. Clark, in a note referring to the concluding sentence above, says, "This must have been '*Non nobis, Domine*,' as no other musical grace was at that time known; the composer also being present." As this is the only evidence adduced by Mr. Clark, in support of the opinion that this grace was composed for, and first sung in the hall of, the Merchant Taylors' Company, we do not think it is sufficient to carry conviction to the minds of others. The question, however, is after all not very important. If we can claim the tune for our William Bird, instead of allowing the honour to pass over to Palestrina, we ought certainly to be content.

No immoral man can be a true patriot, and all those who profess outrageous zeal for the liberty and prosperity of their country, and at the same time infringe her laws, affront her religion, and debauch her people, are but despicable quacks, by fraud or ignorance increasing the disorder they pretend to remedy.—*Nature and Origin of Evil.*

CURIOUS PROPERTY OF THE CIRCLE.

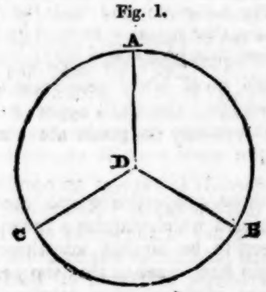
One of the most pleasing problems in practical geometry with which we are acquainted, is that which will occupy our attention in the present paper. It was, we believe, first demonstrated by the late Dr. Hutton, the predecessor of Dr. Olinthus Gregory at the Royal Military Academy, Woolwich.

Those to whom geometry is in any degree familiar, are aware that the "quadrature of the circle," like the "perpetual motion," has been an object of anxious investigation for many ages; the difficulty (and, as has since been demonstrated, the impossibility) of solution seeming still more to increase the desire of effecting it. The squaring or quadrature of the circle, as we have taken occasion to explain in a former paper, is to determine the size of a square whose area shall be *exactly* equal to the area of a given circle. We have, say, a circle one foot in diameter, and we wish to know how many square inches are included in the area of the circle; we may also wish to know what is the length of the circumference of the circle, if extended in a straight line. It is not difficult to ascertain that this last-named length is between three and four feet; and farther calculations show, that, if we express the magnitude by integers and decimals, the length is a trifle less than 3.1416 feet. But what is the nature of this "trifle"? Can we measure it? If we could do so, we could solve the whole question. It is within our power to show that the length is *more* than 3.14159265358979,
but *less* than 3.14159265358980, a difference excessively minute; and we might reduce this difference still more, but we could never annihilate it. We could, in short, never state exactly how long the circumference of a circle is; and consequently we could not determine the area, for one depends on the other.

But though we cannot "square the circle," as it is termed, nor determine exactly the length of its circumference; yet Dr. Hutton's problem enables us to effect something almost as curious, and in one respect still more so. The problem may be put in this form:—"To divide the area of a circle into any number of equal parts, such that all the parts may be equal in area, their peripheries or boundary lines respectively equal, and the periphery of any one of them equal to the circumference of the circle." Here it will be seen are three very curious properties involved; properties which at first sight may appear almost paradoxical.

Let us in the first place suppose that the circle is to be divided into three equal parts. Fig. 1 represents one

Fig. 1.



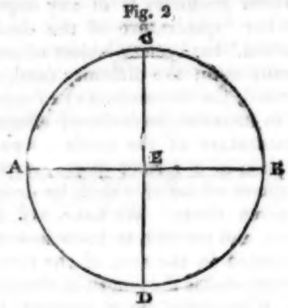
of the most obvious modes of effecting the division, viz., by drawing radii from the centre D to three equidistant points in the circumference, A, B, and C. This divides the circle into three sectors, all equal in area, because the three points are equidistant. So far all is well; but when we come to consider the peripheries, we find the solution wanting in this respect—that the periphery of each part shall be equal to the circumference of the circle.

That the periphery of each part is equal to that of each of the other parts, is plain enough; but it is almost equally plain, even without the aid of geometrical

demonstration, that no one of these divisions has a periphery or boundary line equal to the circumference of the circle. The solution, therefore, fails in meeting the terms of the question.

In like manner, if the circle have to be divided into four parts, and if the same principle is acted on, a similar error will result. We may take four equidistant points in the circumference, as A B C D in fig. 2, and

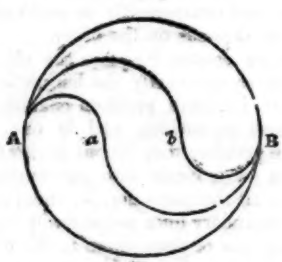
Fig. 2



draw radii to them from the centre E; but although the areas will be equal, and the peripheries also equal, yet the periphery of no one part will be equal to the circumference of the circle.

We will, therefore, instead of effecting the division by *straight lines*, perform it by *semicircles*, combined after a manner which we proceed to explain. We will suppose, in the first instance, that the division is to be into three parts. We suppose a horizontal diameter to be drawn from A to B, fig. 3 and divided into three equal parts,

Fig. 3.



separated by the points *a b*. We then draw a semicircle on the line A *a*, and a larger one on the line A *b*; both above the horizontal diameter. We next draw two semicircles below the diameter, the one on the line *a* B, and the other on the line *b* B. Several obvious consequences result from this mode of division; such as that the two small semicircles are of equal size; the two larger semicircles are also of equal size; the large upper semicircle combines with the small lower semicircle to produce a continuous curved line; the small upper and the large lower semicircles combine to produce a similar result; and the area of the space A *a* B is exactly equal to that of the space A *b* B.

But besides those properties which are obvious to view, there are others which require a little explanation. The figure is seen to be divided into three parts; and we have to explain how it arises that the *peripheries*, as well as the *areas*, of these three parts are all equal. It is one of the laws of geometry, that "the circumferences of circles are as their diameters;" that is, whatever ratio the diameter of a small circle may bear to the circumference, is likewise borne by the diameter of a larger circle to the circumference. If it so happened that a circle one inch in diameter, were exactly three inches in circumference, then the circumference of a circle two inches in diameter would be exactly six inches. But such is not the case; the ratio of the circumference to the diameter is 3.1416, &c. to 1; and this holds true whatever be the size of the circle. As it is true with respect to the whole circle, so is it likewise in the semicircle; and this will enable us to show that the waving line A *b* B, as also the line A *a* B, is equal to the semicircumference of the circle. The straight line A *a* is one-third of the diameter of the circle; and the semicircle drawn on it is, therefore, one-third of the semicircle drawn on A B. In the same way the line *a* B being equal to two-thirds of the diameter of the circle, the semicircle drawn on it is equal to two-thirds of the larger semicircle. Then adding these together ($\frac{1}{3} + \frac{2}{3} = 1$) we find that the waving line A *a* B, formed by the junction of the smaller semicircles, is exactly equal to half the larger circle. The same reasoning exactly will apply to the other waving line A *b* B; whereby we come to the conclusion that each waving line is equal in length to half of the larger circumference; and that, as a necessary consequence, the three portions of the circle are bounded by equal peripheries.

Then, as to areas. The area of a circle does not depend on the diameter in precisely the same way as the circumference; all three of them increase or decrease at at the same time, but not in the same ratio. Thus, the circumference increases *directly* as the diameter; but the area increases as the *square* of the diameter. Consequently if we have two circles, one two inches, and the other four inches, in diameter, the area of the larger one will not merely be *twice* as great as that of the other, but four times, since $2^2 = 4$. It follows that the area of the semicircle on A *a* is only one-fourth of that on A *b*; and if we subtract the former from the latter, we have 3 as the representative of the semicircle A *b* diminished by A *a*, or 6 to represent the centre middle section of the circle. As the whole diameter A B is three times that of A *a*, we have the largest semicircle equal to $3^2 = 9$ times that of the smallest, or the whole area $= 18$ times. Then subtracting the middle portion, whose area is equal to 6, we have 12, which gives 6 as the area of each outer portion. Thus it is shown that the areas of all three parts are equal.

If the circle be divided into 4, 5, or 6 portions, instead of 3, the same principles must be regarded in seeking for a solution, modified according to the mode of division. These three cases are represented in figs. 4, 5, and 6.

Fig. 4.

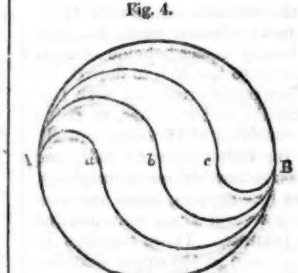


Fig. 5.

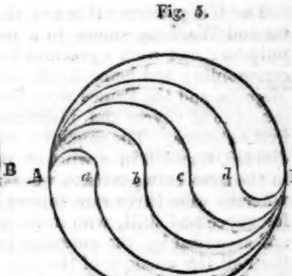
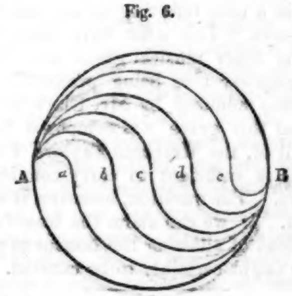


Fig. 6.



In every case the diameter of the circle must be divided into as many equal parts as the circle is to be divided into; and on these small lines, considered as a series of regularly diminishing diameters, semicircles must be

constructed; of which half must be above the diameter and half below it. The upper semi-circumferences meet the lower in such a manner that the two form a series of waving lines, all terminating at the two ends of the diameter A B. If we consider only one half of the entire circle, and bear in mind the principles on which the division has been made, we shall see a very regular and beautiful law pervading the areas. Take, for instance, fig. 6, and regard only the upper half of the circle. We see that this is divided into six portions, all ending in a point at A, but all increasing in area as they extend further from that point. Now if we consider the area of the smallest of these portions, extending only from A to a, to be equal to 1, then the areas of the others will be 3, 5, 7, 9, 11, respectively, forming the odd numbers. This would be the case whether the division of the circle by these waving lines were into 2 parts or into 100; the areas would be 1, 3, 5, &c., according to the extent of the division. If we add up the numbers given above, thus, $1+3+5+7+9+11=36$, we find that the area of the smallest portion is $\frac{1}{36}$th part of that of the entire semicircle which agrees with what was before stated; since A B is six times as great as A a, and $6^2=36$

A little attention to the construction of the problem will show that it admits of the utmost generality in solution. We are enabled, to the fullest extent, to solve these four forms of the problem:—To divide a circle into any number of parts, to make the peripheries of all the parts equal, to make each and every periphery equal to the circumference of the circle, and to make the areas of all the parts equal one to another.

ON THE EXISTENCE OF

TOADS AND OTHER ANIMALS WITHIN BLOCKS OF WOOD AND STONE.

II.

In a former article, we collected a number of instances which have been from time to time recorded, of the existence of toads and other animals imbedded in masses of wood and stone; and we promised to lay before our readers an account of Dr. Buckland's experiments instituted with a view to the establishment or disproval of the fact, how far these animals can exist when excluded from the air and deprived of all means of procuring food.

While we enter our decided protest against such experiments when the subject under consideration is of trivial importance, and little calculated to affect the welfare of man; we must leave with our readers the question how far it is justifiable, to endeavour to remove the very considerable amount of superstition and false reasoning which exists on the subject, when in order to such investigation, the protracted torture and destruction of a number of these harmless reptiles follows as a necessary consequence.

In the month of November, 1825, Dr. Buckland commenced his experiments as follows:—

In one large block of oolitic limestone, from the quarries at Haddington, twelve circular cells were prepared, each about one foot deep, and five inches in diameter, and having a groove or shoulder at its upper margin fitted to receive a circular plate of glass, and a circular slate to protect the glass; the margin of this double cover was closed round, and rendered impenetrable to air and water by a luting of soft clay. Twelve smaller cells, each six inches deep and five inches in diameter, were made in another block of compact siliceous sandstone, viz., the Pennant grit of the coal formation near Bristol; these cells also were covered with similar plates of glass and slate, cemented at the edge by clay. The object of the glass covers was to admit of the animals being inspected, without disturbing the clay, and thus to prevent the admission of external air or insects into the cell. The limestone is so porous that it is easily penetrable by water, and probably also by air: the sandstone is very compact.

On the 26th of November, 1825, one live toad was placed in each of the above-mentioned twenty-four cells, and the double cover of glass and slate placed over each of them, and cemented down by the luting of clay; the weight of each toad in grains was ascertained, and noted at the time of their being placed in the cells; that of the smallest was 815 grains, and of the largest 1185 grains. The large and small animals were distributed in equal pro portions between the limestone and the sandstone cells.

These blocks of stone, were buried together in Dr. Buckland's garden, beneath three feet of earth, and remained unopened until the 10th of December, 1826, on which day they were examined. Every toad in the smaller cells of the compact sandstone was dead, and the bodies of most of them so much decayed, that they must have been dead some months. The greater number of those in the larger cells of porous limestone were alive. No. 1, whose weight when immured was 924 grains, now weighed only 698 grains. No. 5, whose weight when immured was 1185 grains, now weighed 1265 grains. The glass cover over this cell was slightly cracked, so that minute insects might have entered: none, however, were discovered in this cell; but in another cell, the glass cover to which was broken, and the animal within it dead, there was a large assemblage of minute insects, and a similar assemblage also on the outside of the glass of a third cell. In the cell No. 9 a toad, which when put in weighed 988 grains, had increased to 1116 grains, and the glass cover over it was entire; but as the luting of the cell within which this toad had increased in weight, was not particularly examined, it is probable there was some aperture in it by which small insects found admission. No. 11 had decreased from 936 to 652 grains. With the two exceptions just mentioned, the other toads which were alive appeared much emaciated. The death of every individual in the smaller cells of compact sandstone, appears to have resulted from a deficiency in the supply of air, in consequence of the smallness of the cells, and the impenetrable nature of the stone; the larger volume of air originally inclosed in the cells of the limestone, and the porous nature of this stone itself, seems to have favoured the duration of life to the animals enclosed in them without food.

Dr. Buckland notices a defect in these experiments arising from the treatment of the twenty-four toads, before they were enclosed in the blocks of stone. They were shut up and buried on the 26th of November, but the greater number of them had been caught more than two months before that time, and had been imprisoned altogether in a cucumber frame, placed on common garden earth, where the supply of food to so many individuals was probably scanty and their confinement unnatural, so that they were in an unhealthy and somewhat meagre state at the time of their imprisonment. "We can therefore scarcely argue with certainty, from the death of all these individuals within two years, as to the duration of life, which might have been maintained, had they retired spontaneously, and fallen into the torpor of their natural hybernization in good bodily condition."

The results of these experiments amount to this:—all the toads both large and small inclosed in sandstone, and the small toads in the limestone also, were dead at the end of thirteen months. Before the expiration of the second year all the large ones also were dead: these were examined several times during the second year, through the glass covers of the cells, but without removing them to admit air; they appeared always awake, with their eyes open, and never in a state of torpor, their meagreness increasing at each interval in which they were examined, until at length they were found dead: those two also which had gained an accession of weight at the end of the first year, and were then carefully

closed up again, were emaciated and dead before the expiration of the second year.

At the same time that these toads were enclosed in stone, four other toads of middling size were enclosed in three holes cut for this purpose on the north side of the trunk of an apple-tree; two being placed in the largest cell, and each of the others in a single cell; the cells were nearly circular, about five inches deep, and three inches in diameter; they were carefully closed up with a plug of wood, so as to exclude access of insects, and apparently were air-tight. When examined at the end of a year, every one of the toads was dead, and their bodies were decayed.

From the result of the experiments made in the small cells cut in the apple-tree, and the block of compact sandstone, it seems to follow that toads cannot live a year, excluded totally from atmospheric air; and from the experiments in the larger cells, within the block of oolitic limestone, it seems also probable that they cannot survive two years entirely excluded from food:—

We may therefore conclude that there is a want of sufficiently minute and accurate observation in those so frequently recorded cases where toads are said to be found alive within blocks of stone and wood, in cavities that had no communication whatever with the external air. The fact of my two toads having increased in weight at the end of a year, notwithstanding the care that was taken to enclose them perfectly by a luting of clay, shows how very small an aperture will admit minute insects sufficient to maintain life. In the cell No. 5, where the glass was slightly cracked, the communication, though small, was obvious, but in the cell No. 9, where the glass cover remained entire, and where it appears certain, from the increased weight of the enclosed animal, that insects must have found admission, we have an example of these minute animals finding their way into a cell, to which great care had been taken to prevent any possibility of access.

Admitting, then, that toads are occasionally found in cavities of wood and stone, with which there is no communication sufficiently large to allow the ingress and egress of the animal enclosed in them, the Rev. Professor thinks we may find a solution of such phenomena in the habits of these reptiles, and of the insects which form their food. The first effort of the young toad, as soon as it has left its tadpole state, and emerged from the water, is to seek shelter in holes and crevices of rocks and trees. An individual, which when young may have thus entered a cavity by some very narrow aperture, would find abundance of food by catching insects which like itself seek shelter within such cavities, and may soon have increased so much in bulk as to render it impossible to go out again through the narrow aperture at which it entered. A small hole of this kind is very likely to be overlooked by common workmen, who are the only people whose operations on stone and wood disclose cavities in the interior of such substances. In the case of toads, snakes, and lizards, that occasionally issue from stones that are broken in a quarry, or in sinking wells, and sometimes even from strata of coal at the bottom of a coal mine, the evidence is never perfect to show that the reptiles were entirely enclosed in a solid rock; no examination is ever made until the reptile is first discovered by the breaking of the mass in which it was contained, and then it is too late to ascertain without carefully replacing every fragment; indeed the attention of the discoverer is always directed more to the toad, than to any of the minute circumstances as to the state of the cavity in which it was contained,—whether or not there was any hole or crevice by which the animal may have entered the cavity from which it was extracted. Without previous examination it is almost impossible to prove that there was no such communication. In the case of rocks near the surface of the earth, and in stone quarries, reptiles find ready admission to holes and fissures. We have a notorious example of this kind in the lizard found in a chalk-pit and brought alive to the late Dr. Clarke. In the case, also, of wells and coal-pits, a reptile that had fallen down the well or shaft, and survived its fall, would seek its natural retreat in the first hole or crevice it could find, and the miner dislodging it from this cavity to which his previous attention had not been called, might in ignorance conclude that the animal was coeval with the stone from which he had extracted it.

Dr. Buckland remarks on the case of toads that have been said to be found in cavities within blocks of limestone, to which on careful examination no access whatever could be discovered, and where the animal was absolutely and entirely closed up with stone. Should any such case ever have existed (and there seems to be no properly authenticated example of it), it is probable that the communication between this cavity and the external surface had been closed up by stalactic incrustation after the animal had become too large to make its escape. A similar explanation may be offered of the much more probable case of a live toad being entirely surrounded with solid wood. In each case the animal would have continued to increase in bulk so long as the smallest aperture remained by which air and insects could find admission; it would probably become torpid as soon as this aperture was entirely closed by the accumulation of stalactite or the growth of wood; but it still remains to be ascertained how long this state of torpor may continue under total exclusion from food and external air; and although the experiments above recorded show that life did not extend two years in the case of any one of the individuals which formed the subjects of them, yet, for reasons already stated, they are not decisive to show that a state of torpor or suspended animation may not be endured for a much longer time by toads that are healthy and well fed, up to the moment when they are finally cut off from food, and from all direct access of atmospheric air.

The common experiment of burying a toad in a flower pot, covered with a tile, is of no value unless the cover be carefully luted to the pot, and the hole at the bottom of the pot also closed, so as to exclude all possible access of air, earth-worms, and insects. Dr. Buckland has heard of two or three experiments of this kind, in which these precautions have not been taken, and in which at the end of a year the toads have been found alive and well.

Besides the toads enclosed in stone and wood, four others were placed each in a small basin of plaster of Paris, four inches deep, and five inches in diameter, with a cover of the same material carefully luted round with clay; these were buried at the same time, and in the same place, with the blocks of stone, and on being examined at the same time with them in December, 1826, two of the toads were dead, the other two alive, but much emaciated. Dr. Buckland only collects from this experiment that a thin plate of plaster of Paris is permeable to air, in a sufficient degree to maintain the life of a toad for thirteen months.

BY examining the tongue of the patient, physicians find out the diseases of the body, and philosophers the diseases of the mind.—JORTIN.

WHEN Socrates was asked what a man gained by telling lies:—"Not to be believed," said he, "when he speaks truth."

No *cloud* can overshadow a true Christian, but his faith will discern a *rainbow* in it.—BISHOP HORNE.

O LORD! pardon what I have been—amend what I am—and let thy goodness direct what I shall be.—*Found in Manuscripts at Cloyne by* BISHOP BENNET.

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